VOL. VI. No. 20. SEPTEMBER 30, 1909

THE JOURNAL OF PHILOSOPHY
PSYCHOLOGY AND SCIENTIFIC METHODS

ON THE METHODS OF APPLIED MATHEMATICS

THE views expressed in this paper are not consciously derived from those of previous methodologists, but from an examination of the concrete processes of mathematical physics. As they are to that extent original and as, moreover, the form is different from that of the majority of logicians, no useful purpose would be served by reference to works in which it might be possible to discover the germs of some of these ideas.[1]

Nor is this essay in any way connected with the conflicting theories of knowledge or with controversies on the logical nature and philosophy of pure mathematics. Its special feature is the separation of the methodology of applied mathematics from these more speculative considerations, and the result is that the former stands out in a clearer light and attains greater relevance to the actual problems of mathematico-physical investigation.

Whatever may be our opinions concerning the ultimate nature or origin of mathematical reasoning, it would be impossible to deny that it is actually, practically, a series of abstractions. Though the axioms may be necessary, and the reasoning may possess intuitive and absolute validity, this validity is conditioned by the abstractions themselves, and the applicability of the conclusions to any particular existences in the object world is not necessarily implied in the process of mathematical reasoning.

Even in simple cases this can readily be illustrated. The simple arithmetical statement that $2 + 2 = 4$ is a definite unassailable truth of the conceptual order. The concrete application $2d + 2d = 4d$ would be false unless the objects to which it was applied were definite, separable, discrete units which admitted of addition. If, for example, we attempted to apply such arithmetical reasoning to the number of seas or gulfs in the Mediterranean, it would obviously give us a false, meaningless, or invalid result because there is no criterion by which

[1] The nearest approach to an anticipation will perhaps be found in Jevons's "Principles of Science," Chapter XXI. Between that statement and the following there are, however, a number of important differences.

we could say when and where one gulf was contained in or graded into another. The necessary discrete units are nowhere to be found.

It will thus be seen that it is possible, by too close an attention to the conceptual series, to make serious errors in the application of even the most elementary mathematical truths. According to such a strict logical rendering, simple addition is a "pure" mathematical process of absolute conceptual validity, compound addition is a mixed mathematical process dependent on the fact that we actually deal with perceptual discrete units to which the reasoning can be applied. In simple cases of this kind, it would be pedantic to insist on the distinction, but, for logical consistency, it is easy to point out that even here it is valid.

The example of mathematical reasoning in which the conceptual and perceptual elements are most entangled is geometry. This science, dealing as it does with existence under its simplest aspect of extension in space, gives us conceptual truths that are almost universally applicable to the perceptual world and can be verified to a high degree of approximation by experiments with lines drawn on paper. When applied to actual existences, such truths require some small qualifications which are necessitated by the structure of the substances used. The universal applicability of the truths of geometry depends on the perceptual fact that such qualifications are, as a rule, so small as to be practically negligible. Even in geometry, the modern development of fourth dimension and non-Euclidean applications shows that the conceptual series may outrun the perceptual and give us conclusions that have no valid meaning in the object world.

According to this view, the distinction between pure and applied mathematics is one of degree and of convenience. All mathematical reasoning is "pure," and all application is "mixed." In some cases, however, the applicability of the method is so obvious that it can be taken for granted; in others the discussion of the range within which the ideal construction is applicable to concrete instances is a difficult and complicated problem, only to be settled by prolonged and careful experiment, observation, and reasoning.

The starting-point of the present investigations can therefore be summarized in the following three propositions: (*a*) Mathematics is a series of abstractions, a conceptual series of truths, the validity and *a priori* certainty of which is self-contained and conditional on the ideal nature of the construction; (*b*) common knowledge and its outgrowth, experimental science, is a series of truths of a different order. Some may be *a priori,* but most are discovered by the intelligent observation of the object world; (*c*) the inter-relation of (*a*) and (*b*)

is variable, and is dependent on the nature of the abstractions and of the facts of observation and experiment to which they can be applied.

We will now consider our third proposition more closely and see if we can discover any general rules and conditions which will throw light on the nature and necessary limits of this inter-relation. A number of these are immediately apparent. If, for purposes of calculation, we abstract from the totality of our experience certain general properties and work these into our conceptual series, it is obvious that the range within which our concepts admit of valid application depends on the range and certainty of these abstractions.

All Euclidean geometry is built up of a very few of these assumptions, which can be reduced to the fundamental axiom of quantity, Playfair's axiom, and some other self-evident truths. These, according to my own view, are *a priori* in the experience of the race; but, for those who still retain the preevolutionary empiricism of Mill, we can merely say that they are invariably consistent with experience. No one could doubt that for practical purposes their certainty is absolute. So far as the conceptual series includes these and no others, there is no practical limit to the length of the chain of reasoning that can be based on them. At any link in that chain, the conclusion could be verified by actual measurements which only vary from the ideal by the necessary limits of experimental error.

A few, a very few, physical axioms have this same degree of validity. In this class I should place the law of inertia, the conservation of matter (or inertia)[2] which is closely related to the former, and the conservation of energy. These are generally admitted to be of universal applicability, and, so far as mathematical reasoning is based on these, its validity would be as great as that of Euclidean geometry.

One or two other principles, while not *a priori*, possess an empirical validity practically universal. The law of gravitation is perhaps the best example of these, and here again there is no recognizable practical limit to the possibility of application to actual existences. But even here an element of uncertainty will creep in. A calculation based on the continual existence of any portion of ponderable matter would only have approximate validity if carried into the infinities of space and time. It is quite conceivable that matter may evolve from and dissolve into the ether of space. To admit this possibility we must postulate inertia as a property of the ether, but the possibility is there none the less. It is not likely that any actual calculation will be invalidated by such an error, but it well illustrates

[2] Stated in this form, the principle admits the possibility of the evolution and dissolution of ponderable matter.

the empirical nature of mathematical investigations to point out that, at this early stage, we find a limit to its practical certainty.

It will be seen that no series of physical calculations can possess the same range of applicability as ordinary Euclidean geometry. There is no geometry of mass. In theoretical mechanics we have no doubt a conceptual science which, so far as it is based on physical and geometrical axioms, would possess an equivalent theoretical certainty; yet the transference of the conclusions from the conceptual to the perceptual series contains numerous possibilities of error. Outside the limits of statics and dynamics, the validity of physical laws falls off rapidly. All the ordinary generalizations of physical science, laws of radiation with temperature, of conduction of heat, of relation between chemical and physical properties, of electric potential and current, all these, when they are not definitions in disguise, are avowedly empirical approximations applicable to actual existences only within reasonable and recognized limits. Any undue extension of these requires careful and critical treatment, and, if far removed from the possibility of verification, should not be regarded as definite and established truth.

So far we have dealt only with the certainty of the premises, the degree of validity *a priori* or otherwise of the abstractions from which the argument took its origin, and the consequent danger of error from the fact that these premises themselves correspond only imperfectly to actual existences in the object world. Any error on this score would imply a limit to the possibility of concrete applications. But the vast majority of mathematically established deductions not only imply the theoretical possibility of concrete application, but the statement that the abstractions provide an adequate correspondence to concrete reality.

We can readily illustrate this by reference to astronomy. All calculations of gravitational astronomy are not only dependent on the law of inverse squares and the continued existence of an invariable quantity of gravitating matter, but on what we may term a number of existential propositions concerning the actual system to which the mathematics is applied. These calculations also require the disputable and even false premises that the sun and planets are unchangeable in mass, suffering neither loss nor gain; that there are no sources of gravitational force other than the sun and the planets; and that there are no forces other than gravitation which can affect the relative position of celestial bodies. The last in particular is certainly wrong. Gravitational theory will not account for the position of the tail of a comet nor (probably) for the distribution of a spiral nebula, yet these astronomical calculations are the most accurate known to science.

The possibility of an extended series of astronomical calculations depends on the empirical fact of observation that all these sources of error are, for ordinary limits of space and time, so small as to be practically negligible. But, even here, any attempt to calculate the exact relative positions of the planets, say a million years ago or a million years hence, would rightly be received with some considerable degree of scepticism.[3]

All applications of statics and dynamics to actually existing bodies and forces are subject to a similar series of existential assumptions.

It will thus be seen that the possibilities of the interrelation of our conceptual and perceptual series is subject to two necessary limits: (1) That the abstractions on which the reasoning is based have, in a very small percentage of cases, absolute intuitive certainty or intrinsic accuracy; (2) that the concepts are never complete abstractions from reality, and the degree of completeness can only be determined empirically by observation and experiment. Of these two necessary limits, the second is, in normal cases, probably of the greatest practical importance; but both must be noted as theoretical sources of error.

An attempt to calculate the volume of a gas at 1,000 atmosphere by Boyle's law, or to discover the conditions of the atmosphere 500 miles above the earth's surface by the law of diffusion,[4] would undoubtedly come under the first heading. These principles are both empirical laws of limited applicability. A calculation of the age of the earth by secular cooling, or of the position of Halley's comet 5,000 years hence from its present velocity and position and from the gravitation of the sun and other calculable centers, would be invalid from the second cause. The abstractions in each of these cases would be entirely inadequate for the complexity of the problem to be solved.

For the remainder of this article, however, we shall not trouble

[3] I lay no great stress on this suggestion which would require great mathematical analysis to follow to its conclusion; but is it not possible that Leverrier's calculations regarding the periodicity of the variations in the eccentricity of the earth's orbit may be invalid on this account? While his calculations are probably approximate in relation to the last glacial epoch, is it not possible that, through the multiplication of such minute errors as change of orbit due to solar tidal reaction, or gain of mass due to access of meteorites, the greater changes in the eccentricities of planetary orbits may not be periodic at all, but irregular and incalculable. If this suggestion be correct, it may throw light on the geologic problem of the irregular occurrence of glacial epochs, and Croll's hypothesis may be correct in spite of this difficulty.

[4] The law of diffusion depends on the straightness of the path of the individual molecules, which would be curved by gravity if molecular impact were infrequent.

further with these distinctions. It is possible that, if approached from a standpoint less practical than that of the present paper, they might be shown to be part of a larger principle. Whether this be so or not, we shall find that their practical importance will be increased or decreased by similar causes and conditions.

Here is the next step in our investigation. Having discovered two necessary limits to the interrelation of the conceptual series of mathematical abstractions and the perceptual series of facts of observation and experiment, we must now endeavor to ascertain when and to what extent these limits are of definite practical importance. This it is impossible to accomplish completely in general terms. The necessary limits of mathematical investigation vary in incalculable ways with the nature of the special investigation. But, though such an aim can only be accomplished to a limited extent, it is, I believe, possible to lay down general rules, some of which we shall find are immediately applicable to the subject of this essay.

Such general rules will be best elucidated by the critical analysis of special concrete instances in the light of the foregoing principles.

Let us take, as an example, the simple kinematical formula for calculating the distance covered by a falling body ($s = \frac{1}{2}gt^2$). A very elementary knowledge of the facts will show us that this useful little formula will only serve our purpose within a very limited range. Immediately one of the variables is increased beyond a definite limit, a correction must be introduced for the resistance of the air. This, indeed, is so difficult that I doubt whether it has been attempted. But a moderate approximation would imply a complicated differential, including (at least) a variable for the density of the atmosphere, another for the velocity of the body, and (assuming a definite shape) a constant for the ratio between the mass of the body and its surface area.

Let us imagine these difficulties to have been overcome, or the calculation to refer to a planet without an atmosphere. We shall find that, when the distance exceeds a very moderate amount, we need to introduce a variable for the force of gravity which decreases with the square of the distance. Finally, at a height still greater, it is necessary to allow for the perturbing influence of the sun and of other gravitating bodies. In cases of this kind, the practical mathematician relinquishes the problem as insoluble and adopts the short and easy method of finding the theoretical velocity of a particle to infinity.

In this particular instance, it will be seen that the value of our useful empirical formula is, within a very limited range, very great; beyond that, for another limited range, it will answer fairly well, subject to a manageable correction; and finally it breaks down en-

tirely, and the only way of obtaining the faintest clue to what really happens is by a method of approximation.

The errors are cumulative and after a certain limit entirely destroy the validity of the formula.[5]

This example shows us, lying on the surface of our subject, that one factor which limits the applicability of mathematical formulæ to actual entities is the magnitude of the terms and, particularly, of the simple fundamental variables space and time. Whenever a formula is applied beyond the ordinary limits of space and time, its value is doubtful, and if this extrapolation be carried out indefinitely, the applicability is entirely destroyed. While the results are still true as an ideal construction, considered as pure mathematics, they cease to have any bearing on actual perceptual existence.

This property can be illustrated by any physical formula applied to concrete problems. The simple relation between the power and weight in a lever and the distance of each from the fulcrum, will, as a rule, work without serious correction for a bar five feet long. For a bar 50 feet long corrections would be required for the weight of the lever and its lack of rigidity. For a bar 500 feet long, the formula would be useless.

The law of sines is applicable to simple cases of refraction in prisms and lenses, but, though still ideally true, it gives us little assistance in unraveling the interesting natural phenomena of mirage and fata morgana.

Joule's formula, expressing mathematically the first law of thermodynamics, is theoretically (except for the experimentally determined magnitude of J) an allotropic form of that most certain of all physical axioms, the conservation of energy; but it is a truism to say that the limits within which it will express the amount of heat that we can actually obtain in a measurable form, are almost vanishingly small.

From instances like these, which can be multiplied indefinitely, we can infer that, both by reason of the abstractions contained in mathematical formulæ as such and by reason of the complexity of the perceptual world to which they are applied, the applicability of each and every calculation is strictly limited, and, if its terms be multiplied indefinitely, it must necessarily break down and cease to provide any recognizable correspondence with objective fact.

[5] I do not know whether the problem is insoluble, but any one who would calculate the velocity necessary to carry a particle into the sphere of gravitation of the sun, would introduce a large correction into speculations concerning the escape of planetary atmospheres which are now based on the theoretical "velocity to infinity." (Cf. papers by Dr. Johnson Stoney, *Astrophysical Journal*, 1898, and elsewhere.)

In the whole range of mathematical physics, there can not be found a single calculation which can be trusted to be accurate if carried too far from actual objective fact and from the possibility of verification. This indeed is the method of all science. Our abstractions, our calculations, must continually be brought into contact with the new particulars of fact which have been deduced from the abstractions. So far as this is done and so far as this is possible, theoretical questions concerning the necessary limits of mathematical reasoning do not arise in practical work. The limits are plain and obvious and inherent in the character of the work. The existence of facts of observation and experiment by which the theoretical conclusions of mathematical deduction can be tested and verified provides a practical check which effectually prevents undue and illegitimate extensions. It is only when these methods are applied to hypothetical happenings in the far distant realms of time and space, where there is no possibility of direct verification, that the examination of theoretical limits accomplishes, not only the intellectual purpose of aiding the comprehension of the intrinsic nature of scientific method, but the practical, every-day, useful work of providing a wholesome criticism and check on illegitimate speculation which takes upon itself the form and appearance of mathematical precision.

The danger of carrying deduction too far from the sphere of direct verification is recognized in discussions concerning the validity of extrapolation. All prediction is in one sense extrapolation, and all extrapolation possesses a certain theoretical uncertainty; but it is generally recognized that, within certain limits, this method is allowable.

One principle guiding the range of such deductions is found in the difference between rational and empirical formulæ. Extrapolation is much more valid with the former; but, as we have already seen, this distinction is merely one of degree. All but a very few have some elements of empirical or observational uncertainty. The well-known fact that extrapolations are regarded with considerable suspicion by practical scientists, and that such have so often proved mistaken, well illustrates the contention of this thesis.[6] Indeed, this well-known rule can be regarded as merely a particular case of the more general principle, that the applicability of all mathematico-physical formulæ is bound by ordinary limits to the magnitude of the terms, and particularly of those fundamental variables space and time.

[6] Chemists will remember a striking instance of this in the determination of the temperature of liquid hydrogen. This was at first determined by Professor Dewar many degrees wrong by an extrapolation of an empirical conductivity formula.

So far we have been concerned only with simple mathematico-physical formulæ. It now remains to consider the bearing of these principles on more complicated forms of such deductive reasoning.

The complexity of a mathematical formula may be due to one or both of two possible causes. Its complexity may be occasioned by the process of successive approximation. A simple ideal formula, like that representing the fall of a body under gravity, may have added to it a number of corrections in order to enable it to calculate with greater accuracy the actual happenings in a world where such ideal conditions do not prevail. Such a formula bears on the surface evidence of its empirical character, and obviously will not admit of extensive extrapolation.

Another type of complex formula derives its complexity from deduction from more fundamental principles. Its theoretical certainty then depends on the foundations and on the process of deduction. For the range of this type no general rules can be assigned. This can only be determined empirically by a careful examination of its pedigree. To examine this for any particular formula is a problem which belongs to mathematics rather than to methodology.

Leaving this question of theoretical certainty, and confining our attention to practical applicability, it can readily be seen that a complex formula contains sources of error inherent in its own complexity. No detailed acquaintance with practical mathematics is requisite to perceive that every separate variable introduces an additional source of error, and that the error is increased as the grouping of the variables become more and more complex. A slight error in a factor of a complex differential produces a more than proportionate error in the result. It therefore follows that, assuming the same degree of theoretical certainty, the practical value and range of a mathematical formula decreases with each increase of complexity. A complex differential is not inherently superior to a simple formula, but is a useful instrument of investigation where no simple formula is available. Every separate variable introduces a new possibility of error, as does also the more complicated relations of the factors involved. For the purpose of obtaining an approximate solution to such a cosmic problem as the age of the earth, methods dependent on complex differentials are, other things being equal, inferior to those based on simple arithmetic.

At this point the present investigation ends. The fallacies inherent in the illicit mathematical treatment of cosmic problems have been traced to their theoretical source. In so doing, we have glanced at a number of theoretical questions and thrown out from time to time suggestions on which varying stress has been laid. It will now be convenient to sum up the main conclusions.

We have seen that, whatever may be its philosophical meaning, mathematical reasoning is an ideal construction, a series of abstractions, the validity and *a priori* truth of which is strictly limited to the conceptual sphere.

It is also clear that common knowledge and experimental science are a system of truths, largely based on observation and experiment, belonging to a perceptual series of an entirely different order.

All applications of mathematical reasoning to physical problems or to actual existence of any kind are, therefore, a more or less conscious fitting of the conceptual to the perceptual system. This process of fitting is subject to certain theoretical sources of error. These, which are commonly avoided by the continual contact of theoretical deductions with new detailed fact, are of special practical interest when such direct verification is only partially possible. Of these sources of error the following are of special interest: (1) the validity of mathematico-physical formulæ is dependent on the range and theoretical certainty of the physical or other principles involved in the premises; (2) it is also dependent on the completeness with which the conditions relevant to any particular problem have been abstracted from reality; (3) both these sources of error are increased with the complexity of the formulæ and with the magnitude of the substituted terms; (4) in cases of extensive extrapolation, such formulæ are liable to give not only minor errors, but results which bear no recognizable relation to concrete reality.

The essence of a methodological investigation lies in the possibility of concrete application. This little essay is an attempt to bring into closer relations methodology in the abstract and scientific method in the concrete. Though to the philosopher there may in the preceding pages appear to be but little of striking novelty, yet if these simple principles be fearlessly applied, it is clear that they will necessitate the reconsideration of much that has passed current as the outcome of scientific precision. Methods of finding the age of the earth by secular cooling, and much other cosmological speculation, appear in its true light as doubtful theory. Indeed, all acquainted with the multitudinous applications of mathematical reasoning to the complexity of the universe around us can, for themselves, find countless other examples.

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MAY A REALIST BE A PRAGMATIST?

III. The Implications of Psychological Pragmatism[1]

IN the second article of this series we endeavored to formulate the implications of "biological pragmatism" or instrumentalism, the first of the four types of pragmatic doctrine which were set forth in the introductory paper on the definitions of realism and pragmatism. We found that the instrumentalist theory of the origin of thought definitely implied the realistic point of view, and that in itself it afforded no support to a subjectivistic interpretation of either the proper method or the proper scope of cognitive activity. We have next to consider the implications of

Psychological Pragmatism.—This view, which was also entitled the motor theory of truth, asserts that the *truth* of a belief is identical with its *verification,* or—the *being* true of a belief is identical with the process of experiencing or *finding* it true.

It is the same thing for a belief to be true and for a belief to be verified, *i. e.,* experienced as true? Here for the first time we come upon a form of pragmatism that is on its face definitely anti-realistic. For both pragmatist and non-pragmatist admit the truth is a relation of agreement between a belief and a reality. The psychological pragmatist declares that this truth relation of agreement consists in being experienced. Its *esse* is *percipi.* No belief can be true antecedently to its being proved, for, by definition, truth equals proof or verification. Realism maintains, as we have seen, that the reality of a thing does not consist in its being known. And this applies equally to specific facts and to relations between those facts. A *belief* in the roundness of the earth was a fact in the minds of some of the ancients. The roundness of the earth is now itself admitted to be a fact. There is and was the relation of agreement between the belief of some of the ancients and the reality. Consequently, we say that the belief was true. Now, is it possible to regard this relation of agreement as non-existent prior to the time when it was verified by the circumnavigation of the globe? Only a subjectivist can so regard it. But, it may be answered, what does this relation of agreement between belief and reality *mean,* except the successful leading of the belief to the immediate experience of the thing believed? To which we may in turn reply: How could a belief lead successfully to the thing believed, unless it had been in some sort of agreement with it beforehand? A key proves to be the right key because it fits into the lock. Is it just chance that we can fit it into

[1] For the two preceding papers of this series, see this JOURNAL, Vol. VI., pp. 460–463 and 485–490.

the lock? No, there is a reason for it. And the reason is that the conformation of the key corresponds to the conformation of the lock. The pragmatist never seems to raise the question *why* some beliefs lead us successfully to our facts and others do not. And yet the need of raising this question and the kind of answer it requires are both so obvious. The belief does not lead up to the fact by chance, or for no reason, any more than the key fits into the lock by chance. The reason the belief leads up to the fact is because the relation preexisting between it and the fact is like the relation preexisting between the key and the lock, namely, a relation of conformity or correspondence. This may appear more plainly from the following illustration: Two travelers desirous of reaching an inn as quickly as possible come to a fork in the road. A declares his belief in the easterly road being the shortest. B disagrees and declares for the road westerly. Deciding to test their theories pragmatically, they separate. B reaches the inn first and asserts triumphantly that the successful issue of his belief proves its truth. It turns out, however, that on the road taken by B there was a man with a wagon who gave B a lift, while A's road was solitary. Did the successful leading of B's belief constitute its truth?

Now, I suppose that the pragmatist will laugh at this little tale as a sample of the way in which an intellectualistic criticism can miss the point. I should quite agree with him as to the injustice of my illustration if intended as a *reductio ad absurdum* of his position. But the real point that I wish to bring out is the ground on which the pragmatist would condemn the example as unfair. He would doubtless say that it was not B's belief itself which led to his reaching the inn first, but rather the fortuitous and perfectly irrelevant fact that the wagon happened to be on his road, and that consequently the belief in that road being the shortest had not been tested at all. Such a reply, however, reveals a tacit assumption with which the pragmatist qualifies his definition of truth as the leading of a belief to a successful result. The successful consequences constituting the truth of a belief must be due to the nature of the belief and not to any outside or adventitious circumstances connected with it. And now in the light of this qualifying assumption we may ask again: How could a belief be of such a nature as to lead to its own successful confirmation in immediate experience except by being in antecedent agreement with the objects immediately experienced?

I imagine that the pragmatist may at this point interrupt somewhat impatiently with some such remark as the following: We will grant that there is a sense in which a belief or judgment must be in agreement with reality prior to its verification in experience, but

the only way to describe that antecedent conformity is to describe it as the possibility of verification. The potentiality of verification is, if you insist, the antecedent truth of an idea from which its actual verification results. Opium produces sleep because of something antecedently present in its nature, but that antecedent property of opium can only be described as its dormitive virtue. To say that opium produces sleep because of its soporific virtue is not any more enlightening than to say simply that it produces sleep. It is, then, at best nothing more than a verbal and barren victory which we have gained over the psychological pragmatist in forcing him to admit a distinction between truth and verification. Now, this reply of the pragmatist has considerable force. Just as Mill may be said to have made no substantial advance beyond Berkeley and Hume when he defined a material object as a permanent possibility of sensations as distinguished from the mere intermittent actuality of those sensations, so we shall have made no substantial advance beyond the identification of truth and verification if we are forced to define our antecedent truth as merely the possibility of verification. In short, the pragmatist extends a perfectly fair and honorable challenge to his opponent either to define the truth relation in some other way than in terms of a verification-experience (actual or possible) or else to confess himself beaten. I think that this challenge, fairly given, may be as fairly met—that it is possible to describe in independent terms the actual nature of the relation between a belief and a reality which makes possible the future verification of that belief. But before undertaking to do this concretely and specifically, I should like to remind the pragmatist of a general point in this connection which has been often made, and never, so far as I know, refuted. *There is no such thing as a mere possibility or potentiality.* A is never the possibility of B except in virtue of possessing also some actual nature of its own. The egg is not a bare potentiality of a chick. It is also an egg, which is something just as actual as the chick of which it is the potentiality. The soporofic potentiality of opium consists of certain quite actual intramolecular relations, the specific nature of which it is the business of the physiological chemist to ascertain. So, also, the potentiality of the verification experience must be a certain actual relation between the thing believed, and the thing that exists; and it is now squarely up to us to define the specific nature of that relation. I think that this can be done very simply, as follows:

We know that it is possible for one thing to stand in two or more different relations—to have membership in two or more relational contexts or systems. One man can be president of the United States and also a hunter of big game. One bottle can occupy the center

of a table and also contain ink. One number, 30, can be 3×10 and also $28 + 2$, etc., etc. When one thing is considered now in one context and again in another context, we make the judgment of identity. "A is identical with B" means that there is a something, *m*, which in the context α is called A and which in the context β is called B. Because A and B denote one thing in two contexts they are said to stand to one another in the relation of identity. Now consider any thinkable or experienceable *content, e. g.,* "land westward of Europe." This thing or content can occupy two relational contexts. It can have (and no one doubts that it does and did have) membership in that single, infinite, and all-inclusive, totality of spatially and temporally related contents which we call "the existing world," or "the world of facts." It could have (and no one doubts that it did have) membership in the system of "things believed by Columbus." *When one content is the object of a belief and is also a thing that exists, there subsists between the content as believed and the content as existing that particular form of the relation of identity which is called* TRUTH. To say that a belief when true "corresponds" to a reality, means that the thing believed is identical with a thing that exists. Columbus believed that there was land westward of Europe. His belief was true for the reason that what he believed happened to be a fact. When we believe in a thing that is a fact, our belief is true. When we believe in a thing that is not a fact our belief is false. So much for the definition of truth. How, then, is verification related to it? One thing can stand in the belief context and also in the existence context without that duality of context being experienced. But it is equally possible for the duality of context to be itself the object of a single experience. This last is verification. *When a thing is experienced by a person to be both a fact and a thing believed, then the belief in that thing is* VERIFIED. Before America was discovered Columbus's belief in land west of Europe was a true belief, because it was a belief in a fact. But it was not a verified belief, because no one had experienced land west of Europe as both an object of existence and an object of belief. Because the belief was true and also because Columbus had courage to take steps to test it, it came to be verified. The thing that Columbus believed was at last experienced by him as an existent fact. The belief was not made any more true by being proved; it would have continued true whether America had ever been discovered or not. Its truth constituted, to be sure, the possibility of its being verified, but we have answered the pragmatist's challenge to describe that antecedent truth relation as something in itself actual and not as a mere potentiality of verification. Indeed, when confronted with the statement that the truth of a proposition is the same as its proof,

one is tempted to remind the pragmatist of a caution always given to the students in formal logic. I mean the caution against confusing the failure to prove a proposition true with the disproof of its truth. If truth were identical with verification we could disprove the truth of a conclusion by simply showing that the premises adduced in support of it were insufficient to prove it.

The only way, so far as I can see, for the psychological pragmatist to justify his identification of the truth of a belief and the experience of proving it true, is for him boldly to maintain the subjectivistic interpretation of the relation of identity. Does the identity of a believed thing and an existent thing have its *esse* in *percipi*? Is it impossible for two things to be identical except when they are experienced as identical? Does the perception of agreement between belief and reality create the agreement perceived? The psychological pragmatist must answer these questions in the affirmative. What justification there is for ascribing to our experience the extraordinary power of creating relations of identity between the facts experienced, I, as a realist, am unable to understand. Why (barring illusions) should two things be perceived as identical unless they were identical?

Now when one is unable to find any adequate logical evidence in support of a theory, one is irresistibly tempted, in the interest of a clearer understanding, to seek the extra-logical causes of its origin. I believe that there are two factors which have been largely operative in leading pragmatists to identify truth and verification. One of the great props of modern subjectivism consists in the failure to distinguish between the *ratio essendi* and the *ratio cognoscendi*. Berkeley, in one of his dialogues [I have not the text at hand and must rely on memory] imagines himself to question a peasant as to why he believes in the existence of his cherry tree; the peasant is made to reply that he believes that the tree is real because he can see it, touch it, and taste its fruit. This is taken by the author as implying that the plain man, when uncorrupted by a realistic pseudo-philosophy, instinctively identifies the material world with his sense-perceptions. Because he perceives the cherry tree, he says it is real. To be sure, but he means by this that his tactual and visual perceptions are the *ratio cognoscendi* of the tree, not at all its *ratio essendi;* more simply, they are the cause, not of the tree's existence, but only of *his belief* in the tree's existence. The confusion stands out clearly enough in the following case: We believe that it has rained during the night because we perceive in the morning that the road is wet. The perceived wetness of the roads is the *ratio cognoscendi* of the rain during the night. To say that it was its *ratio essendi,* to ascribe to the *wet look* of things in the morning the power of creating what happened the night before would seem absurd enough. And yet a

very considerable number of the arguments of Berkeley and also of his successors, both German and British, are embodiments of this same confusion. Would the pragmatist regard the attempt to distinguish sharply between *ratio cognoscendi* and *ratio essendi* as one of the verbal and barren subtleties of intellectualistic logic? I do not know. But certain it is that if he would consent so to regard it, his own thesis could be established finely. For it is undeniable that the verification of a belief is our only ground for knowing it to have been true; it is its *ratio cognoscendi* and if that means its *ratio essendi* also, why then verification is what constitutes truth.

The second factor that may serve to explain the rise of psychological pragmatism rests on a consideration that was mentioned in the first article of this series. Pragmatism is primarily a protest against absolutism, and the polemics of the pragmatist are directed so exclusively against the absolutist that the possibility of there being a position different alike from absolutism and pragmatism is usually overlooked. The absolute idealist conceives of truth as a fixed and unalterable system of ideas, contained in an eternal consciousness, whose proper nature remains forever above and beyond human experience. With considerable skill the pragmatist attacks this conception as an assumption that is at once gratuitous and useless. The inaccessible truth contained in the mind of a hypothetical absolute is surely not our human truth, nor can it by definition ever be used as a check for our human errors. This absolutistic truth seems to be the only kind of *objective* truth that the pragmatist recognizes, and the only alternative to his own conception. To define truth as a certain type of human experience seems preferable to defining it as something that we can never experience at all. It never seems to occur to the pragmatist that not every one who conceives truth objectively finds it necessary to invent an absolute as a safe hiding place for that truth. Truth may be, as we have seen, purely objective and self-existent and yet a perfectly accessible and experienceable relation of identity between things that are believed and things that exist. And I submit that there is a certain naïveté in assuming (as I think the pragmatists do assume) that a demonstration of the greater futility of the absolutist conception of truth is an adequate proof of their pragmatic substitute for it.

The answer to our question, May a realist be a pragmatist of the "psychological" kind? must be an unqualified negative. The theory that identifies truth with those experiences of *successful leading* by means of which truth is sometimes verified appears to be false, and it is certainly anti-realistic, for it makes the *esse* of the truth relation consist in its *percipi*.

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DISCUSSION

DR. MONTAGUE AND THE PRAGMATIC NOTION OF VALUE

IN controversy, it is important to tell the truth, but it is still more important to confute the opponent. Sometimes, consequently, the opponent is in all innocence confuted at the expense of the truth; for the eagerness to make a point shuts out much, foreshortens much, and prevents a man from seeing anything but what he wishes. I am afraid that in Vol. VI., No. 9, of this JOURNAL, Dr. Montague, in his article on "The True, the Good, and the Beautiful from a Pragmatic Standpoint," has placed himself in this too common situation of the controversialist. So eager does he appear to have been to distinguish between "the true, the good, and the beautiful," that he has become blind altogether to a certain common character they happen to have, and appears to have missed the point that pragmatists identify truth and beauty as "mere forms of goodness" because of this common character, not because of any divergencies they may contain. That they contain divergencies no pragmatist has ever denied. What the pragmatist has denied is the notion that their excellence consists in those divergencies; what he has insisted on is the fact that their value is derived from a common function symbolized by the word "good," just as an oyster-shell, a steel blade, and a flat stone are "knives," through the exercise of the common function of cutting. But this thesis Dr. Montague has not considered at all.

To establish his contention he has manufactured a new statement of the "pragmatic standpoint." He calls it "the disposition to reinterpret the logical, ethical, and esthetic values of experience in the light of their relations to the life-processes of the organism." It sees "human experience as a series of efforts to bring about a harmonious adjustment or vital equilibrium between the private experience of the individual and the incomparably broader experience of an environing nature." This adjustment may be attained in three ways: "First, the (individual) element may undergo whatever alteration of its nature is demanded by the context, the context itself remaining unaltered; or second, the context may undergo whatever alteration is demanded by the element, the latter remaining unaltered; or third, the element and its context may each of them spontaneously, and without compulsion from one another, attain harmony or equilibrium." The first of these adjustments Dr. Montague identifies with truth, "for when we are testing the truth of a judgment it is essential to the success of the

process that we make the judgment accord with the environing facts." The second he identifies with "goodness," the satisfaction of desire; which occurs "when the environment conforms to or gratifies the conative demands of the individual." The third he identifies with beauty, which "simply happens" in the accord of the environment "with the organism or with any part of it, in such a way as to accelerate or facilitate its processes."

Now it might easily be urged against the positions here taken up that Dr. Montague is putting up a man of straw in order to knock him down. If he were really to confute the pragmatists, he must confute them out of their own mouths. There are pragmatic accounts of truth galore with which Dr. Montague's account is in no sense compatible; there are pragmatic accounts of goodness and beauty with which his are little more harmonious. Properly, it was his business to show that pragmatic accounts of truth are not consistent with such accounts of goodness and beauty. It was not sufficient merely to assert, in a historical account of the relation of Dr. Schiller to Mr. Bradley, that the "conative and the cognitive types of value are as distinct from one another as north and south, and to seek to identify them or to reduce either to a form of the other is sheer confusion." It was not sufficient; and it is still less sufficient to have admitted in the same paragraph that "no experience is so purely conative as not to have a cognitive aspect, and none is so purely cognitive as to be free from the element of conation" (p. 236). If Dr. Montague seriously meant to refute the pragmatic thesis that the true and the beautiful are "forms of the good," it was his proper task to show that the pragmatist's own proof of this relation was incompatible with its existence. It was his business to confute them out of their own mouths, not to manufacture his own version of what they mean.

But I am content to accept this version at its own face value and to show that the man of straw, in falling, must needs tumble on its author's head. It must do so because its author, in his eagerness to show the differences in values, failed to consider a certain other difference which the pragmatists have insisted on, and the discussion of which is as old as Plato. That difference is the difference between instrumental and final goods. As pragmatism has made much of the instrumental aspect of value, it may be well to consider a little, what, according to Dr. Montague's pragmatism, the instrumental good consists in. The business of life, "human experience," Dr. Montague tells us at the outset of his paper, "may be viewed as a series of efforts to bring about a harmonious adjustment, or vital equilibrium, between the private experience of the individual and the incomparably broader experience of an environing nature."

Such an adjustment or "vital equilibrium," if attained, would be a stopping point, uninstrumental, self-sufficient, intrinsically stable. It is the goal of effort, the poise of judgment, the condition of pleasure. Now clearly, if the "series of efforts" have any value, it is a value they derive from their success in establishing this adjustment. The excellence of an instrument, though Dr. Montague's discussion does not allow it, is determinable traditionally by its success in fulfilling its purpose. And this purpose, enunciated so unfalteringly by Dr. Montague—is it good or bad, desired or undesired? Dr. Montague does not say, he is too busy (having told us that human experience is a *series of efforts* to bring about a "vital equilibrium"), proving that some efforts differ from others. He forgets to observe that they are all *efforts,* and that they are united in a *common* purpose. He fails to note that this purpose, this "vital equilibrium" is, on his own rules, to be attained *conatively,* and that the *seat* of the process of alteration is not in issue—that what is in issue is the fact that the attainment of what he calls "truth" is a *process,* just as the attainment of what he calls "good" is a process. When either of these is attained, there occurs what he calls a "vital equilibrium," but he insists that there is nothing in common between these equilibria, simply because the ways of getting at them are different. Yet strangely enough, they *are* equilibria, they *are* "harmonious adjustments of organisms to environment." Under the circumstances one would be tempted to say that the intrinsic nature of goodness was identical with the "vital equilibrium" and that the "truth-process" and the "desire-process" and the "beauty-process" were instruments that bring it about. But for some occult reason Dr. Montague supposes that because the organism changes more than the environment, the equilibrium at the end of the change is different from that attained when the environment changes more than the organism. Similarly it may be argued that if Dr. Montague travels to Boston by way of New Haven, and I travel to Boston over the Harvard Bridge, and Dr. Schiller is translated to Boston on a magic carpet, just "happens" in Boston, that we have arrived at different cities. The way through New Haven is the way through New Haven, and the way over Harvard Bridge is no doubt the way over Harvard Bridge, and the carpet is the carpet, but they are all *good* only as *ways to* Boston, and the purpose in hand, being-in-Boston, is good-in-itself. It is a "vital equilibrium." The instrumental value of things is definable only by their efficiency in realizing their purpose; their goodness is derived from that purpose, itself an excellence, intrinsic, immediate, simple. If truth and goodness and beauty are processes only, purposing a vital equilibrium, they are "good" in so far as

they reach it; and each is *good in the same way* as the others; if they *are* the "vital equilibrium," it is incumbent on Dr. Montague to show that this differs *intrinsically* for each form of value. But he can not do this—for *qua* "vital equilibrium" truth and beauty and goodness are identical.

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REVIEWS AND ABSTRACTS OF LITERATURE

Principii di scienza etica. F. DE SARLO & G. CALÒ. Palermo: Sandron (no date). Pp. 316.

La patologia mentale in rapport all' etica e al diritto. By the same authors. Palermo: Sandron. Pp. 194.

It is certainly not easy to unite the idea of ethics as a science, which implies the principle of causation, and the idea of the indetermination of the will. And the difficulty can be excluded only by giving to the word "science" an extended meaning. The authors describe science as "the orderly and systematic knowledge of a given order of facts, of their laws and of the ultimate principles of these laws" (p. 1). The object of ethical science is the moral experience; and this consists in the fact, which can be immediately experienced, of the consciousness of every individual of civil society in the present degree of civilization that certain actions are considered as deserving to be done, or as obligatory, and certain others as unworthy, or contrary to duty (p. 5). The moral consciousness is clearly distinguished from the social consciousness, because this last does not necessarily imply the sentiment of the intrinsic value of a certain form of conduct; and it differs also from the juridical, political, religious, and esthetic consciousness. Consequently, ethics is an independent science, independent also of metaphysics. There is a metaphysics of morals, but no metaphysical ethics (p. 28). Ethics is founded on an immediate, evident certainty, while metaphysics, which is a rational reconstruction of the world, can not pretend to this evidence and immediate certainty. The latter has to do with *what is,* while the first has for its object *what ought to be.* The judgments of the one are what the Germans call *Existentialurtheile,* those of the other, *Werthurtheile.* Ethics is not a merely descriptive, but an essentially normative science (p. 29). The special question of the normative character of ethics is an aspect of the general question of the nature of the highest functions of the mind. In a certain sense, it can be said that the mind is in itself normative as knowing, as creating, as willing: in theoretical, in esthetical, in practical activity, it has the idea of an end to which it ascribes a value in itself, which it considers as normal and suiting to its nature, and which consequently reveals itself under the form of norms. But it must be considered that norms exist only in reference to certain ends, and ends exist only, in the mind, in reference to the will (p. 34). So that only moral laws are truly normative.

In our inquiry after those absolute values—those ultimate ends, which

do not admit of any causal explanation, and are the logical *prius* of every other valuation—we must follow two methods: the psychological, and the historical. The psychological analysis shows that in the experience of value, which characterizes the moral consciousness, we have a synthesis of knowledge, feeling, and will (pp. 76–9). Rationalism in its various forms (metaphysical or theological rationalism, naturalistic and utilitarian, or idealistic rationalism), voluntarism, and sentimentalism (the theory of sympathy, and the Scotch intuitionalism), are one-sided and insufficient explanations of the moral consciousness. The fact of valuation is the highest and most complicated fact of conscious activity, and implies the cooperation of all the fundamental functions of mind. The moral experience is something quite original and not reducible to anything else: it is a spontaneous reaction of our consciousness in presence of certain objects which appear to us as valuable or the contrary. What distinguishes the judgment of valuation in general from the theoretical judgment, is the fact that in the last subject and predicate are two objects of thought independent of any subjective action; and what characterizes especially the ethical judgment is the consciousness of our freedom to realize or not the moral values (p. 83). A peculiar character of the moral consciousness is that the valuing capacity itself can become an object of moral valuation and that it is capable of perceiving the differences of value—differences which are *qualitatively* distinct from the apprehension of the particular values.

In the moral consciousness we can distinguish a *contemplative* and an *active* element, and we pass from the one to the other by means of the feeling of obligation, in which it is impossible to say if it is the consciousness of the intrinsic value of the object which prevails, or the consciousness of our free will. For what is ethically approved is also at the same time apprehended as a more or less urgent obligation for our will (p. 111). As to the sense of responsibility, it is originally a manifestation of the pressure which the collective consciousness exercises on the individual consciousness; but as soon as the individual recognizes that the social sanctions have no intrinsic value, he becomes the judge of his own actions and feels responsible before his own conscience (p. 118). Free will, as the capacity of free choice, of *unmotived* adhesion to one of the motives proposed to our mind, is a necessary postulate of our ethical consciousness. By this *unmotived* choice the authors mean that there must be a moment in which the cause determining the will to choose a motive rather than the other can be found only in the power of initiative of the will itself (p. 130).

This sounds very much like the *arbitrium indifferentiæ* of the schoolmen, and like the ancient doctrine of the separate faculties of the soul, and does not seem quite consistent with what the authors themselves say about psychical and moral experience, which they consider as a synthesis of reason, feeling, and will, and with the idea of the *ego* as a spiritual substance, as an original principle of activity (p. 137). A substance must be something determinate; an activity must tend to certain ends. An absolutely indeterminate activity means simply chance. An uncaused volition, a capacity of acting without a motive, is inconceivable, and if it

existed, would be destructive of morality, which implies the idea of certain ends. No doubt we are conscious of being free, but freedom consists in the power of reflecting before acting, of conceiving and choosing between different ends of conduct. Not determinism, but indeterminism, excludes freedom. The question is not if our actions are caused or uncaused; the question is, what is the nature of the causes which determine them? If amongst these causes there is my reason, my consciousness of myself as conceiving ends and trying to realize them, I am free.

The history of morality—which occupies the second part of the work, and is mostly a résumé of the able work by Professor Westermark on "The Origin and Development of Moral Ideas"—can be described as the history of the gradual assertion of human freedom, of the realization of man's moral nature. The authors treat successively of moral evolution in respect of the valuation of the intrinsic qualities of the human personality (intelligence, culture, courage, force, temperance, etc.), of justice, of benevolence; and they consider this historical treatment as verifying the psychological analysis, for it shows, in their opinion, the universality and necessity of the fundamental ethical principles. But even if it could be shown that in the course of civilization some valuations are constant and unchangeable, one does not see the use of such an inquiry, which ought to be included in the field of sociology rather than of ethics. If the principles of conduct are, as the authors pretend, evident in themselves, history is useless.

On the contrary, historical inquiry shows the erroneous side of the authors' point of view. It shows that ethics is not, as is commonly supposed, a so-called normative science, nor a science of *Werthurtheile*. The facts and laws of moral life are the object of a theoretical science, like every other. All theoretical sciences have applied sciences corresponding to them, and so also there is a theoretical and an applied science of ethics. But before establishing the norms, it is necessary to establish the ends of conduct. Before studying what ought to be, one must study what is. Nor is the judgment of valuation a distinctive character of moral science. Everything in the world can be made an object of valuation. And every *Werthurtheil* implies an *Existentialurtheil*, that is to say, the existence of an object to which we ascribe a value. Before approving an act of justice or of charity, or condemning an act of injustice or of inhumanity, we must know what is justice and charity, injustice and inhumanity, precisely in the same way that to appreciate a piece of sugar or avoid a poison, we must know that the one is good for our organism and the other dangerous. The only difference is in the degree of importance.

The little work on "Mental Pathology in Relation to Ethics and Law" is an attempt to determine the normality of conduct and the ideas of imputability and responsibility. There are three kinds of finality and normality: physiological, psychological, and ethical. Normality of conduct implies the existence of a will and an end, and some knowledge of reality, and of the means of realizing the end, and implies also a certain coordination and subordination of the ends, consequently a regular organization of psychical life and of the actions by which the will manifests

itself, a continuity, a coherence (pp. 4–5). So it is possible to distinguish between those forms of delinquency and immorality, which depend on the various forms of abnormality and degeneration, and those forms which have an extra-pathological origin; *i. e.,* depend on free will. Determinism (evolutional, psychological, sociological) is destructive of responsibilty; it confuses pathological abnormality with ethico-juridical abnormality. It would be useless to discuss these theories, which are those of the old spiritualistic school and of the classical school of penal law. The fact is that not determinism, but indeterminism, is destructive of responsibility. How could a man be made responsible for an unmotived action? Nor is the idea of punishment at all irreconcilable with determinism. Do not we punish children, although not even the authors would pretend that they are responsible? In a certain sense, freedom is a necessary presupposition of morality, responsibility, and punishment. Freedom is the clear consciousness of one's own acts and of their consequences, and the power of understanding the justice of punishment and of feeling its effects. Punishment is justified in so far as it appears not only as a necessary social reaction against the doings of the criminal, but also as a means of educating him, of making of him a better man, of producing in him the consciousness of true freedom. For, doubtless, only the moral and honest man is truly free.

In this last work one finds here and there mistakes which appear the more strange as one of the authors has begun his career as a doctor. *E. g.,* at page 57, it is asserted that hysteria and epilepsy are among the *factors* productive of degeneration. It would seem rather that they are the *results* of a degeneration of the organism, or the form of the degeneration itself. At page 44 neurasthenia is considered as the common germ, from which originate all mental illnesses. But are there not diseases of the brain having a traumatic origin, and other causes independent of any degeneration of the nervous system? And must all people with hysterical, neurasthenic, and hypochondriac symptoms, be shut up, as the authors advise (p. 63), in lunatic asylums! Indeed, it seems absurd to consider that nervous sensibility, which is often the only cause of those symptoms, and can not be said to present any real pathological character, as implying a fatal, irremediable, constitutional degeneration.

One last remark. The authors criticize the idea of a semi-responsibility, which they consider as a juridical monstrosity (p. 152). Such an idea is certainly a monstrosity in their indeterministic theory of the will. Between free will in the traditional sense and no will at all there is no mid-way. But it is quite justifiable in a deterministic theory. A semi-responsible person is a person who, although subject to pathological impulses, is susceptible to moral education and strengthening of the will, and to the effects of punishment. Such a person can not be left exempt from punishment, like a completely irresponsible criminal, who has no consciousness of his doings and no power to control his impulses; and, in justice, can not be so severely punished as a completely responsible person who, acting in the full power of his intelligence, and with the clear insight of his motives and his objects, and after mature deliberation, shows a

much greater perversion of the will and badness of character than a neuropathic individual who for a moment loses control of himself and commits a crime. As there are various degrees of freedom, so there must be various degrees of responsibility; in the same way that there are different and more or less severe punishments attaching to different and more or less grievous offences.

All this remains unintelligible in the indeterministic doctrine of pure free will.

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René Descartes. Eine Einführung in seine Werke. K. JUNGMANN. Leipzig: Fritz Eckardt Verlag. 1908. Pp. viii + 234.

We have here a careful and painstaking account of the philosophy of Descartes, written with German thoroughness and based not only upon his philosophical works, but also upon the recent edition of his letters. The exposition is clear, the criticism sound, and both together make the book a valuable addition to the long list of commentaries upon Descartes and his school. The principal objection to be brought against it is rather of a literary than of a philosophical nature. The style is heavy and the reading may easily become a task. However much students of Descartes may feel repaid for the time devoted to the book, it is not likely to attract those not already interested in the subject. Perhaps the charm of a clear and simple style is too much to ask of a writer upon philosophy; certainly it is more than one usually gets from him, and Herr Jungmann is no more careless than many of his fellows.

The arrangement of the book is based both upon chronology and subject-matter, a combination always to be desired and, in the case of Descartes, particularly successful. A chapter upon method is followed by others upon mathematics, epistemology, and the sciences, under which captions are placed metaphysics, physics, psycho-physics, ethics, and religion. The account of Descartes's achievements in mathematics is excellent. Their nature, their value, and their relation to the rest of the mathematical thought of that century are all admirably set forth, while, at the same time, it is constantly urged that at bottom Descartes was never a mathematician. He was not interested in mathematical truths as such, but occupied himself with them because of their relation to philosophy. This is undoubtedly true to a large extent, though it seems to me one is hardly justified in saying that at no time in his development did Descartes have an interest in mathematics for its own sake. Such an interest was doubtless temporary and was promptly subordinated to others which were more permanent, but for a brief period it seems to have really existed.

The chapter upon epistemology occupies by far the larger portion of the book and contains many of the most distinctively cartesian doctrines. These are treated with considerable detail, but without much attempt to evaluate them. In opposition to various other students, the author regards Descartes's position as critical rather than as dogmatic, because

he begins with an unprejudiced examination of the knowing faculty, and having once reached definite results, he does not attempt to transgress the limits thus established. In like manner his idealism is pronounced to be of the empirical type, because he regards knowledge of the external world as impossible except as the result of the direct action of outer objects. All this elaborate structure of epistemological theory, however, can not be regarded as an end in itself. Like mathematics it was of value in the eyes of Descartes only because of its relation to physics, which is the aim and center of his philosophy. For him epistemology was a propædeutic to physical philosophy, and he cared for no other metaphysics.

The basal conceptions of the cartesian physics are the vortex and the ether. From these two principles the whole material universe, both inorganic and organic, can be explained. The details of the explanation are naturally more fully worked out in some directions than in others, and Herr Jungmann follows Descartes in this respect. The book is brought to an end by a chapter upon Descartes's writings, which are divided into three groups: the published works, the fragments, and the letters. The last are of especial value in tracing the development of Descartes's thought and the relation of his books to one another. The letters make it possible to state with accuracy the order in which many of Descartes's theories took shape in his own mind, and, in some cases, show that the previous conjectures upon this subject are altogether wrong. In fact not only here, but throughout the book, Herr Jungmann pays a good deal of attention to fixing exact dates in connection with his author's philosophical activity. The care with which this work has been done, united with equal caution in the formation of conjectures where sufficient evidence is lacking, will undoubtedly make the monograph of value for reference to any student of philosophy desiring exact information concerning the origin of the Cartesian theories.

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JOURNALS AND NEW BOOKS

REVUE PHILOSOPHIQUE. September, 1909. *L'explication scientifique et la causalité* (pp. 225–254): Kozlowski. – The idea of causation as irreversible dependence is indispensable and results from the architectonic need that compels our understanding to reconstruct reality in accordance with the demands of logic. *L'esthétique scientifique* (pp. 255–267): Ch. Lalo. – The introduction to *Les sentiments esthétiques* which is about to appear. *Des vicissitudes de la lutte pour la vie* (pp. 268–275): Georgesco. – Man is a simple spectator of the drama resulting from the unalterable essence of the living principle. *Notes et discussions. Religion et mysticisme, d'après l'observation psycho-pathologique:* Duprat. *Il n'y a pas de logique formelle:* Hourticq. *Analyses et comptes rendus.* Berquer, *La notion de valeur:* J. Segond. C. Piat, *Insuffisance des philosophies de l'intuition:* M. Solovine. Ewald, *Kants kritischer Idealismus:* J. Segond. Gaultier, *L'idéal moderne:* J. Segond.

Marinsary Leblond, *L'idéal du XIX^e siècle:* OSSIP-LOURIE. Spiller, *Papers on Moral Education:* J. SEGOND. Elsee, *Neoplatonism in relation to Christian Philosophy:* C. HUIT. P. Barth, *Die Stoa:* C. HUIT. Bové, *Sistema cientifico Luliano:* F. PICAVET. Bonilla y San Martin, *Historia de la filosofia española hasta el siglo* XII: J. PÉRÈS. Lacombe, *Taine, historien et sociologue:* L. ARRÉAT. Rudler, *La jeunesse de B. constant:* L. DUGAS. Lederbogen, *F. Schlegels Geschichtsphilosophie:* J. SEGOND. Spranger, *W. von Humboldt und die Humanitatsidee:* J. WOLF. Bants, *Gesamelte Schriften:* J. SEGOND. Kühn, *Kants Prolegomena in sprachlicher Bearbeitung:* J. SEGOND. *Revue des périodiques étrangers.*

REVUE DE METAPHYSIQUE ET DE MORALE. July, 1909. *La logique de l'infini* (pp. 461–482): H. POINCARÉ. – The paradoxes of the transfinite can be avoided by a psychologically correct method. We should (1) consider only objects definable by a finite number of words, (2) make all propositions concerning the infinite traductions from propositions about the finite, (3) avoid classifications and non-predicative definitions. *Les sources néocriticistes de la dialectique synthétique* (pp. 483–500): L. DAURIAC. – A discussion of the relations between the philosophies of O. Hamelin and Renouvier. *Correspondance inédite de Ch. Renouvier et de Ch. Secrétan (suite)* (pp. 501–551). – A continuation of the personal and philosophic correspondence which has appeared in previous numbers of this journal. *Études critiques. Identité et réalité, par E. Meyerson:* A. REY. *Discussions: A propos d'Auguste Sabatier. Reponse de M. G. Sorel:* H. MONNIER. *Questions pratiques. La démocratie et la loi d'après deux ouvrages récents:* G. AILLET. *Supplément.*

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NOTES AND NEWS

IN the *New York Medical Journal* for September 11 there is an interesting account of the new Henry Phipps Psychiatric Ward at Johns Hopkins. We quote the introductory statement of the article: "It is a far cry from the stone-walled dungeons, the manacles and the whips, the repugnant food, and the generally misguided and cruel treatment of the lunatic a century ago to the psychiatric ward about to be added to the Johns Hopkins Hospital, Baltimore, through a munificent endowment by Mr. Henry Phipps. Through the courtesy of Mr. George Atterbury, the

architect charged with the preparation of the plans, we are enabled to present the plans of three of the floors, with a description of the more important features of the building." This description is comprehensive and includes an account of the portions of the building devoted to the several interests that are gathered under the one roof. To quote from the article: "The general functions of the building comprise research, educational and clinical departments, and an out-patient department, and these are kept distinct. The clinical department is divided for general and private patients; the entire top floor, separated into suites of varying sizes, is devoted to the latter."

IN *La revue du mois* for August, M. Georges Bohn contributes an article on "L'éxperience en biologie et en psychologie comparée." In the main it is a sketch of the leading ideas contributed to the development of biological science by Cuvier, Bernard, and Giard. It concludes with the citation of a number of experiments which tend to show that instincts are not fixed, but variable, and that the variation depends on the "physiological states" of the organism. The production of these states under artificial conditions appears to be attended by characteristic instinctive reactions, and these appear to be independent of the normal instinctive reactions of the organisms concerned. For instance, males exhibit the characteristic reactions of females, and females of males. M. Bohn urges that the use of the experimental methods of physiology should be the important concern of students of comparative psychology, especially in the study of instinct.

AT the recent second decennial celebration of the opening of Clark University honorary degrees were conferred upon the following persons: the degree of Doctor of Laws upon Percival Lowell, of Boston, Dr. Ernest Fox Nichols, president of Dartmouth College, Professor Franz Boas of Columbia University, Dr. Adolf Meyer of Johns Hopkins University, and Professor L. William Stern of the University of Breslau; the degree of Doctor of Physics upon Professor Albert Abraham Michelson of the University of Chicago, and Professor Ernest Rutherford of the University of Manchester, England; the degree of Doctor of Letters upon Professor Edward B. Titchener of Cornell University.

WE quote from the *Athenæum* the following note: "Professor W. J. Ashley is preparing for Messrs. Longmans a student's edition of John Stuart Mill's "Principles of Political Economy." He will indicate, with their dates, all the changes in the text which show any variation or development in Mill's opinions. There will be an introduction, setting forth the position of Mill in the movement of economic thought; and an appendix will give references to subsequent writers."

THE Open Court Publishing Company has in press Arthur Collier's "Clavis Universalis," edited with Introduction and Notes by Ethel Bowman, M.A., Wellesley College. The book was published in the same year —1713—as Berkeley's "Dialogues between Hylas and Philonous," and is a careful and spirited argument for an idealistic conception of the universe. Curiously enough, Berkeley and Collier seem to have been entirely uninfluenced, the one by the other.

FRANCIS GALTON has recently prepared a volume of reminiscences entitled "Memoirs of My Life." On the last birthday of King Edward the degree of knighthood was conferred upon the great scholar in recognition of his long service to science. Sir Francis Galton is one of the small group of living men who still represent the Victorian era. The group includes also Hooker, Wallace, Avebury, Lister, and Huggins.

DR. ARNOLD RUGER, who has charge of the annual bibliography of philosophical publications, instituted by the Fourth International Congress of Philosophy last September, will be glad to receive copies of papers bearing on philosophy, psychology, logic, ethics, and esthetics. These papers should be sent in care of the Weiss'che Universitäts-Buchhandlung, Heidelberg, Germany.

PROFESSOR ERDMAN, of the University of Bonn, will take the place at the University of Berlin of the late Professor Paulsen. Professor Erdmann's position at Bonn will be filled by Professor A. Külpe, of the University of Würzburg. Professor Külpe will institute at Bonn a laboratory of experimental psychology modelled on the laboratory at Würzburg.

A BIBLIOGRAPHY of philosophy and pedagogy has been organized under the editorship of A. F. Formiggini at Modène. It contains among the discussions of *Questione filosofiche* papers by D. Calò and by Vailati; and (in preparation) "Il materialismo dialettico e il materialismo storico di Federico Engels," by Rudolfo Mondolfo.

THE volume entitled "Allgemeine Geschichte der Philosophie" in the collection "Die Kultur der Gegenwart" contains the following essays of interest: "Die Anfänge der Philosophie und die Philosophie der primitiven Völker," by William Wundt, and "Die neuere Philosophie," by William Windelband.

ON the occasion of the recent Leipzig celebration Dr. Wilhelm Wundt, the eminent psychologist, who made the principal address, was given the title of excellency. He was also made an honorary citizen of the city of Leipzig.

DR. IRVING KING, who has for the past two years held the position of assistant professor in education at the University of Michigan, has been appointed assistant professor in education at the State University of Iowa.

PROFESSOR J. MARK BALDWIN has been made Docteur en Science, causa honoris, of the University of Geneva.

PROFESSOR G. H. HOWISON has been made professor emeritus of philosophy at the University of California.